

be sorry to lay down a hard-and-fast rule. The order of educational exposition is not necessarily identical with that of scientific discovery. Facts are more readily remembered if the principle connecting them is known beforehand. And if a teacher, by offering a few preliminary generalities, can peptonise a rather indigestible mental nutriment, why should he, on purely *doctrinaire* grounds, be forbidden to do so? Did Dr. Morley ever read a novel, and if so, did he never yield to the human temptation of anticipating the course of the narrative by looking at the end?

Dr. Morley has distributed the general reactions throughout the text, calling attention to them, however, by printing them in spaced type. The theoretical sections are as a rule brief, but to the point. An excellent account of the benzene theory is introduced; but we would point out that Hübner has given a simpler proof of the existence of the symmetrical *meta* pair of hydrogen atoms than that of Wroblewsky quoted by Dr. Morley: this proof is based upon reactions of the two nitro-salicylic acids.

The selection of facts is judicious, and, inasmuch as Dr. Morley's plan demanded that each compound should be treated of with considerable fulness, the student can hardly turn to any section of the book without learning all that is of real importance under that head.

Several inaccuracies have crept into the book, although they are not sufficiently numerous to interfere seriously with its usefulness. In his preface Dr. Morley acknowledges his indebtedness to Beilstein's treatise in his search for facts, and we fear that he has not always been sufficiently careful to ascertain how far Beilstein's statements are traversed by more recent experimental results.

Thus on p. 136 the author introduces glycerin ether—formerly dear to classification as the only example of the ether of a trihydric alcohol. But Tollens and Loë have shown that, whatever this compound may be, it is certainly not an ether of glycerin.

On p. 389 it is stated as a universal rule, that, in the conversion of diazo-compounds into substituted azo-compounds, "where nitrogen becomes attached to an atom of carbon in a benzene nucleus, the nitrogen takes up a para-position with regard to one of the groups already present." Mazzara, Witt, Liebermann, and Griess have shown that the nitrogen may also take up the ortho-position.

Following Beilstein the author has altered Wertheim's formula for conhydrine, $C_8H_{17}NO$, into $C_8H_{19}NO$. This has of course been done in order to bring Wertheim's statement, which Dr. Morley gives, that conhydrine may be broken up into water and coniine, into harmony with Hofmann's formula for coniine. But Hofmann has shown that Wertheim's conhydrine formula is correct, and that it is his experimental fact which is wrong: conhydrine does not yield coniine. In this connection it is strange that Dr. Morley makes no mention of Ladenburg's synthetic optically-inactive coniine (*a*-isopropylpiperidine).

Under piperidine (p. 434) Königs's assertion that pyridine can be reduced to this compound by treatment with tin and hydrochloric acid is given. Dr. Morley must have overlooked Ladenburg's criticism of this work.

In the indigo group we find isatin (p. 386) represented as a lactam instead of as a lactim, and the so-called nitroso-oxindol (in reality isotoxim) formulated as a true

nitroso-compound, instead of as an isonitroso-compound. It is of course conceivable that in these two cases Dr. Morley does not share the views put forward in Baeyer's later work on the indigo-compounds.

In the foregoing instances the information is, as already stated, merely not up to date. But there are one or two statements in the book, the source of which we are quite unable to trace. Thus we are told (p. 339) that "anthraquinone forms a compound with bisulphite of soda." If there is one thing that distinguishes anthraquinone from the quinones of the other hydrocarbons with complex nuclei—from phenanthraquinone, chrysoquinone, &c.—it is the fact that it does *not* form a compound with bisulphite of soda.

Again, under the head of ultimate analysis of organic compounds, we read:—

"Many mixtures have been suggested from time to time as substitutes for oxide of copper; the latest is a mixture of potassic chromate and precipitated binoxide of manganese proposed by Dr. Perkin."

Dr. Perkin would indeed have much to answer for if he had proposed such a substitute for oxide of copper. The mixture was proposed as a substitute for *reduced* copper, to absorb the oxides of nitrogen formed during the combustion of nitrogenous organic compounds.

The equation for the action of trichloride of phosphorus on acetic acid (p. 47) is an instance of the strange vitality which symmetrically-constructed and plausible but quite erroneous chemical equations sometimes display. We do not blame Dr. Morley for introducing the equation: it is given in all organic text-books, ancient and modern, from the time of Gerhardt to the present day, and will probably continue to be employed, translated into the notation of the distant future, at a time when our present formulæ have become as unintelligible as cuneiform inscriptions. The correct equation may however be found, by the curious in such matters, in a paper by Dr. Thorpe, (*Chem. Soc. Trans.*, 1880, p. 186), who was at the trouble to work out the reaction quantitatively.

An excellent feature, unusual in an elementary work of this kind, is to be found in the copious references, designed to encourage in students the habit of reading original papers for themselves.

F. R. JAPP

THE SPRINGS OF CONDUCT

The Springs of Conduct; an Essay in Evolution. By C. Lloyd Morgan. (London: Kegan Paul, Trench, and Co., 1885.)

THIS is a thoughtful and extremely well-written little book on psychology and ethics, regarded from the standpoint of evolution. There is not much in it that is strikingly original; but the material is so well arranged, and the views so lucidly expressed, that the work constitutes a most interesting epitome of modern thought upon the subjects of which it treats. The author is a man well informed as to his facts, while his ability as an analyst may be remembered by the readers of this journal, in the pages of which it was well displayed a year or two ago in a criticism upon the work of the present writer. On that occasion Mr. Morgan took exception to the study of animal intelligence and mental evolution in animals, on the ground that it is impossible to obtain any verified

knowledge of the psychology of brutes, seeing that we cannot directly interrogate them upon the nature of their feelings or mental states. The discussion which followed appears to have had the effect of somewhat modifying his original views; for these, as now stated in his book, are not so severely sceptical as they were when stated in these columns. That is to say, he now appears to recognise the possibility of comparative psychology as a science, although its subject-matter is necessarily restricted by the inadequacy of our "ejective" knowledge of animal intelligence.

We are in such full agreement with the whole essay that our only criticisms upon it refer to matters of comparative detail. These are as follows:—

Mr. Morgan gives it as his opinion that we cannot conceive of matter apart from motion (p. 94), for, in order to do so, we should require to conceive of matter as absolutely cold, "and of such absolutely cold matter we have no knowledge." The fact, however, that we have no knowledge of absolutely cold matter is no proof that we are not able to conceive of matter as absolutely cold. The so-called absolute zero of temperature surely admits of conception as definite as it would were it possible to take an actual reading of its occurrence.

Mr. Morgan's use of the word "instinct" appears to us equivocal. At one time instinctive actions are expressly affirmed to mean adaptive actions of an involuntary and unconscious kind (pp. 226-7); while at another time it is said "Mr. Darwin clearly shows that the satisfaction of any instinctive emotion carries with it a subdued form of pleasure; while, on the other hand, if those instinctive emotions be not satisfied, there results a still more marked feeling of uneasiness, which is a subdued form of pain" (p. 259). Now, clearly, there can be no such thing as an unconscious emotion, an unconscious form of pleasure, or a "still more marked [unconscious] feeling of uneasiness." Mr. Morgan thus appears to have fallen into the inevitable confusion which is the fate of all writers who fail clearly to distinguish between instinct and reflex action, or expressly to include the former term within the territory of consciousness. For these reasons we cannot follow the author's analysis where it leads up to the conclusion that volition is coextensive with consciousness (p. 226 *et seq.*). We may be conscious of the sudden anguish of neuralgia: can it be said that this consciousness is due to, or accompanied by, any act of volition? Mr. Morgan would answer that with the pain there arises a desire that it should cease (p. 229). But, in the first place, a desire is not a volition; and, in the next place, even the desire has here no time to arise before the pain is past.

In one place where Mr. Morgan refers to the views of the present writer, he represents them as differing from those of Dr. Bain, while in reality no difference obtains. First, he quotes the following passage from "Mental Evolution in Animals":—

"What is the difference between the mode of operation of the cerebral hemispheres and that of the lower ganglia which may be taken to correspond with the great subjective distinction between the consciousness which may attend the former, and the no-consciousness which is invariably characteristic of the latter? I think the only difference that can be pointed to is a difference of rate or time, which clearly implies that the nervous mechanism

concerned has not been fully habituated to the performance of the response required. . . . Reflex action may be regarded as the rapid movement of a well-oiled machine, consciousness as the heat evolved by the internal friction of some other machine, and psychical processes as the light which is given out when such heat rises to redness. Consciousness is but an adjunct which arises when the physical process, owing to infrequency of repetition, complexity of operation, or other causes, involves what I have before called ganglionic friction."

Now, on this passage Mr. Morgan remarks that he does not consider such ganglionic friction so important a factor in the evolution of consciousness as is "the diffusion of nerve-disturbance" enunciated by Dr. Bain. But surely the former principle includes the latter. For it is only due to this internal friction that the diffusion of nerve-disturbance can be supposed to take place. If all the paths of nervous discharge were freely open, the nervous disturbance would course rapidly and easily along the habitual channels, with comparatively little diffusion as a result. It is only in cases where no one set of paths are more readily open than other sets that alternative directions are offered to the flow of nervous disturbance, with diffusion as a result. The resistances thus encountered—or the ganglionic friction thus created—finds its measurable expression in the delay of eventual response. But although ganglionic friction may arise from such "complexity of operation" (so leading to diffusion), it may also arise from "infrequency of repetition or other causes." Therefore the term ganglionic friction includes all that is expressed by the term diffusion, and differs from it only in being more comprehensive, or in recognising other conditions of cerebral action leading to consciousness, the occurrence of which is always expressed by delay.

GEORGE J. ROMANES

OUR BOOK SHELF

Spectrum Analysis. Six Lectures delivered in 1868 before the Society of Apothecaries in London. By Sir Henry E. Roscoe, F.R.S. Fourth Edition, Revised and Considerably Enlarged by the Author and by Arthur Schuster, Ph.D., F.R.S. (London: Macmillan and Co., 1886.)

THIS is a fourth edition of a well-known book, and the joint authors have evidently taken some trouble to bring the present edition up to date. To this end, the arrangement of the book, which is rather peculiar, lends itself very well. The peculiarity of the arrangement to which we refer is this. At the time that the lectures were first delivered, now nearly twenty years ago, the literature of the subject was so restricted that Prof. Roscoe found it easy and convenient to reinforce the subject-matter of each lecture by reprinting, immediately after it, the particular memoirs on which it had been based. Hence the first edition was a very precious boon to two classes of people: there was an excellent popular account of the new science, and there were the complete memoirs conveniently brought together for those who wished to go more deeply into the subject.

In the present edition an attempt has been made, as we have said, to bring the lectures more or less up to date, and considering the volume of the work which has been done since 1868, one can understand that this has been no easy task. When we pass, however, from the lectures to the appended memoirs so much cannot be said; indeed the interest of this part of the book is now chiefly antiquarian, if we except reprints of Dr. Schuster's own papers, which are given, we believe, *in extenso*,